XXXVI. *Reasons for dissenting from the Report of the Committee appointed to consider of Mr.* Wilson's *Experiments; including Remarks on some Experiments exhibited by Mr.* Nairne. *By Dr.* Musgrave, *F. R. S.*

Read June 25, 1778.

I DO not find that Dr. FRANKLIN, in any of the passages where he speaks of the efficacy of sharp-pointed conductors to prevent electrical explosions, has expressed any doubt of their being universally preferable for this purpose to those which have a blunt or spherical termination. The same observation may be made of the other gentlemen who are the advocates for his doctrine. It may therefore be assumed, that both he and they mean to assert an universal proposition, "That "sharp points will, in all cases, draw off the electrical "fluid silently within the distance at which rounded "ends will explode; or, at least, that the former sort will "in no case receive an explosion at a greater distance "than the latter." I think it necessary to observe, that, though I dissent from this doctrine, I do not mean to assert the contrary universal proposition, but only to deny the universality of that asserted by Dr. FRANKLIN, which I appre-

I apprehend to be sometimes true, and sometimes also false.

But before I attempt to specify the particular cases in which the sharp and the blunt terminations are respectively more liable to electrical explosions, it may be of use to shew (what many gentlemen seem not to be thoroughly aware of) that sharp points having the most perfect communication with the earth, are not wholly exempt from receiving them. My first authority shall be Dr. FRANKLIN himself. "Let a person," says he, p. 60. "standing on the floor, present the point of a needle at "twelve or more inches from it [the prime conductor], "and while the needle is so presented, the conductor "cannot be charged, the point drawing off the fire as "fast as it is thrown on by the electrical globe. Let it "be charged, and then present the point at the same dis-"tance, and it will *suddenly* be discharged." The word *suddenly* means, I suppose, that it will receive an explosion; that being the most natural and obvious proof of the *suddenness* of the discharge. The same thing is more directly asserted by Mr. HENLY, in vol. LXIV. of the Phil. Transf. p. 138. where he informs us, that in discharging three of his large jars, to the coating of which he had connected a wire nicely tapered to a point, the fire flew to the point, and the jars were discharged with a full and loud

loud explosion. A third, and equally decisive, proof is furnished by Mr. NAIRNE's own experiments, though seemingly made with a contrary view. For when the double or interrupted conductor was used, and the second conductor fixed down by screws at about three inches distance from the first, the point presented to the contrary end of the second conductor was found to receive a strong and loud explosion, with a white light at the distance of at least three inches.

If we compare this experiment with another, very common one, exhibited at the same time by Mr. NAIRNE, the comparison will, perhaps, lead us to the discovery of a principle upon which electrical explosions very frequently depend. Though the point, in the circumstances above described, received so strong an explosion, yet when it was presented directly to the prime conductor, it received no explosion whatever at any distance, unless a succession of weak sparks, at the distance of about a quarter of an inch, can be called so. To what must this difference be attributed? Plainly to the different quantity of electric fluid accumulated on the prime conductor in the one and the other case. Where the point is presented to the prime conductor, from the time the machine begins to work, the property which is attributed to them, and which, in some cases, they really possess, of stealing

away the electricity silently; this property, I say, operating from the very beginning, prevents the electric fluid from being accumulated in the prime conductor, and of course the quantity of it will always be small. But when a double or interrupted conductor is used, the second conductor receives no electricity till the prime conductor is pretty highly charged, and, if put at the greatest striking distance, not till it is fully charged, and consequently the sharp point presented to the opposite end can carry away none of it till that time; when the whole quantity is thrown off at once. It should seem then, that the explosion in one case, and the non-explosion in the other, depended wholly upon the different quantities to be thrown off: whence it will follow, that though a small quantity of electricity will pass off silently upon a point, yet that this power is very limited; for that if a somewhat greater quantity be applied suddenly to a sharp point, it will not pass off silently, but create an explosion in proportion to its density.

The facts above related are a sufficient answer to that other experiment of Mr. NAIRNE'S, in which he exhibited a sharp point, that when perfectly communicating with the earth drew off the electricity silently from the prime conductor; but received explosions freely, when the communication was broken by interposing little isthmuses of sealing wax. This experiment, it is true, demon-

demonſtrates that a broken communication will occaſion the ſharp point to receive an exploſion; and ſo far it muſt be owned to be concluſive. But if it was intended to ſuggeſt, that whenever ſharp points do receive an exploſion, it is owing to this circumſtance, and, conſequently, that Mr. WILSON's experiments at the Pantheon were unfairly made; in this view it has no weight, becauſe we have already ſeen that an interrupted communication is not the only circumſtance that will produce an exploſion, for that increaſing the quantity of electricity will have the ſame effect.

I cannot omit the opportunity here offered me, of remarking the unfairneſs of the inſinuations that have been thrown out to the prejudice of Mr. WILSON. Had there been any juggle in making his experiments, it would certainly have been detected by the committee appointed to examine them. And in caſe of ſuch a detection, it was the duty of the committee to lay open the impoſture both to the Society and the Public. Inſtead of which, inſtead of diſputing or even doubting the fairneſs of them, they have in a manner admitted it, by only ſaying in their report, that they appear to be inconcluſive. This, I ſay, is admitting the facts to be fairly ſtated: unleſs we could ſuppoſe their regard for Mr. WILSON, and tenderneſs for his reputation, had induced

them, after detecting the fallacy of his experiments, to pafs it over in filence; of which improper partiality I do not know that they are fo much as fufpected. What therefore the committee, after a ftrict fcrutiny of the matter, did not think themfelves warranted to fay, I take for granted they would not infinuate; and that therefore fuch infinuations can only arife from the levity of more obfcure perfons, puzzled perhaps by the feeming contradiction between Mr. WILSON's experiments and thofe of Mr. NAIRNE, and too impatient to inveftigate the real caufes of that difference.

I am perfuaded, however, that the known property of fharp points to carry off electricity filently, when the quantity is fmall, together with that other principle, which I apprehend I have here eftablifhed, that they ceafe to do fo when the quantity is large; that thefe two (taken together) will clear up the whole difficulty, and account for Mr. NAIRNE's experiments, without any impeachment to thofe of Mr. WILSON. I have already had occafion, in the courfe of this argument, to confider two of thofe experiments, of which therefore I fhall fay no more; but proceed, without further digreffion, to examine thofe that remain.

The firft I fhall mention is that, in which the prime conductor, being previoufly charged with electricity, a fharp

ſharp point is preſented to it within the attracting, and without the exploding diſtance, and then brought ſlowly on towards it. In this caſe no exploſion follows; neither is there any reaſon to expect it ſhould, becauſe the quantity of electricity is gradually diminiſhed by the approach of the point, ſo that when it comes within the ſtriking diſtance there is not enough left to make an exploſion.

It is equally eaſy to explain what happens when a tranſverſe arm is hung ſo as to oſcillate freely upon the prime conductor, and two equal cylinders of tin-foil are ſuſpended, one at each end of this arm in perfect equilibrio with each other. The apparatus being in this ſtate, if the machine be worked, the two cylinders will remain ſtationary, neither of them aſcending or deſcending. They will alſo remain ſtationary, if a point be preſented to one, and a rounded end to the other. In the firſt caſe they are electrified, but remain motionleſs, becauſe there is no conducting body within the ſphere of their action. In the ſecond, the reſult is the ſame; becauſe, making in fact part of the prime conductor, the point preſented to one of them prevents any accumulation of electricity. When the point is withdrawn, and the rounded end ſuffered to remain, an accumulation takes place, becauſe there is nothing now to ſteal it away; and the conſequence is, that the cylinder deſcends towards the rounded end, and explodes

explodes as foon as that accumulation arrives at a certain period.

Thirdly, when an intermediate conductor is ufed, terminated at each end with a ball, and the middle of it refting in equilibrio upon a pivot, on which it has a free ofcillation upwards and downwards; if in this ftate a point is placed under the end moft diftant from the prime conductor, the machine being then worked, the other end will approach fo near the prime conductor, as that the ftream of electricity will flow freely into it, as faft as it is produced by the action of the wheel. In this cafe there will be no explofion; and the reafon is obvious, becaufe the fecond conductor, when it approaches fo near the firft as to form an uninterrupted channel for the electric ftream, becomes virtually a part of the firft. Hence the point operates upon both together, juft as it does when prefented directly to the prime conductor, that is, it fteals away the electricity by little and little, leaving not enough to give an explofion. When inftead of the point a polifhed ball is placed under the fame end as before, this being lefs difpofed to receive the electric fluid, conveys away none of it; fo that accumulating to a certain degree upon the prime conductor, it explodes upon the contiguous end of the fecond, which, having a free ofcillation, flies up with the ftroke, and carries the

oppofite

oppofite end towards the ball, where, being faturated, it gives a fnap; the recoil of which fnap throws that end up, and the contrary end back towards the conductor, and fo on alternately, as long as the machine continues working.

The event, however, is widely different when the fecond conductor, inftead of having a free ofcillation, is fcrewed down in one place, and at fuch a diftance from the prime conductor, as not to receive the electric fluid till confiderably accumulated. For then the fharp point, previoufly oppofed to its other end, difcharges it, as was before obferved, not in a continued ftream and filently, but at intervals, and with a ftrong explofion.

The laft of Mr. NAIRNE's experiments, and the only one yet unconfidered, is that of the fharp point, which, being fixed to a kind of inverted pendulum, ofcillated with great velocity under the prime conductor, without receiving any explofion. Now from this experiment I do not comprehend how any general conclufion can poffibly be drawn. It has been already fhewn, from the acknowledgement of Dr. FRANKLIN, and the experiments of Mr. HENLY and Mr. NAIRNE, that electricity, accumulated to a certain degree, will explode upon a point. If, therefore, in any particular inftance it does not explode, what can we infer from it, but that the accumulation in

 every

every ſuch inſtance was not ſufficiently great; which may happen either from the ſmallneſs of the apparatus, or from want of care in making the experiment.

And now if we look back upon Mr. NAIRNE'S experiments (which, by the by, have not all of them the merit of novelty) we ſhall find them to be nothing more than different exemplifications of this well-known principle, that ſharp points giving leſs reſiſtance to the ingreſs of the electric fluid will draw it off at a greater diſtance than blunt or ſpherical terminations, and where the quantity is ſmall will draw it off ſilently. This, I ſay, is the whole amount of his experiments; the only one of them in which the electric fluid had time to accumulate, being attended with a different event from the reſt, and producing, as might reaſonably be expected, a ſtrong exploſion.

It is not, however, this ſingle property of ſharp-pointed conductors, which muſt decide the queſtion. We have already ſeen, that there are two properties inſeparable from them, both of which muſt be taken into the account, before we can determine the propriety of affixing them to buildings, particularly powder magazines, as preſervatives from lightning: firſt, their greater propenſity to admit the electric fluid, in conſequence of which they act upon electrified bodies at a greater diſtance than rounded

rounded ends will; and, ſecondly, their incapacity to draw away more than a certain quantity of electricity without an exploſion.

The firſt quality enables them, when electricity is accumulated gradually, or when they are brought gradually towards the electrified body, to ſteal away the fluid by little and little, till there is not enough left to give an exploſion. And hence, in common experiments, the point, placed at a greater diſtance than the ball, will prevent the electricity from exploding, as it otherwiſe would do, upon the latter. But if we combine this quality with the ſecond, the ſuperior propenſity to admit, with the incapacity in certain circumſtances of diſcharging ſilently, it will be evident, *à priori*, that the phenomena muſt in ſuch caſes be reverſed, juſt as they appear to be in Mr. WILSON's experiments; that the point muſt ſtrike at a greater diſtance, and the rounded end at a leſſer.

What puts this matter beyond a doubt is, that when the double or interrupted conductor is uſed, the experiment may be ſo managed, as that the ball ſhall receive an exploſion at a greater diſtance than the point, or the point at a greater diſtance than the ball, at the pleaſure of the operator. If care be taken, at the beginning of the experiment, to ſet the ſecond conductor at the greateſt

 diſtance

distance from the first, compatible with its giving a full and smart explosion, the point in that case will receive the explosion at a much greater distance than the ball. I was myself once present at an experiment, when the difference was as 6 to 1, that is, the ball would receive no explosion at a greater distance than $\frac{7}{8}$ths of an inch, when the point received it at six times that distance. On the contrary, if the second conductor be put considerably within the distance above described, the ball will receive an explosion much farther off than the point. Upon repeating both these experiments lately with a small machine, I found the result to be as follows. When the distance between the first and second conductor was $1\frac{5}{8}$th of an inch, the point was struck at $2\frac{1}{16}$th of an inch; but a ball of $1\frac{5}{8}$ths of an inch in diameter would not take the stroke at more than one inch. But when the distance between the conductors was only $\frac{5}{8}$ths of an inch, the point could not be struck at more than $\frac{7}{8}$ths, whereas a ball of the same diameter as before was struck at 7 inches and $\frac{3}{4}$, and a lesser ball $\frac{3}{10}$ths of an inch in diameter at 6 inches and $\frac{3}{16}$ths. I have been told also, but have not yet had time to verify it, that a medium distance may be found, at which if the second conductor be set, the point and ball presented to the other end will be exactly upon a par with respect to the exploding distance.

These

Thefe phenomena, to perfons who have not carefully confidered them, muft appear fo extraordinary, that unlefs the caufe of the diverfity is explained, they will perhaps be led to fufpect fome unfairnefs in making the experiment. The truth, however, is this; that when the two conductors are fet at the greater of the two diftances, the abfolute quantity of electricity collected before the explofion is exactly the fame in each experiment; and therefore the diftances of the ball and point from the fecond conductor being equal, and the greateft at which either of them will be ftruck, the explofion will go to the point, as being more fufceptible, and giving lefs refiftance than the ball. But in the fecond fuppofed cafe, when the fecond conductor is fet confiderably within the former diftance, the quantity of electricity which explodes upon the point and the ball is not the fame; the point in this cafe exerting its known property of ftealing away the electricity filently, which the ball from its greater refiftance is incapable of doing. The confequence is, that the quantity accumulated to give an explofion upon the ball is greater than that which explodes upon the point, and being greater will very naturally explode to a greater diftance.

I might fafely have refted the matter upon this ground; but another proof, equally decifive, having fince

occurred, it is but doing justice to my argument to insert it here. In the experiments made with a view to settle this dispute, and published by Mr. HENLY about four years ago, there is one, the fifth of that set, which it is difficult to reconcile with the doctrine here laid down, that electricity strongly accumulated, and moving with great velocity, will explode upon a sharp point rather than a ball.

He describes it thus: " Having insulated the jar, and " connected *by chains* with the external coating, on one " side a knob, and on the other side a sharp-pointed wire, " both being insulated and standing five inches from " each other, I placed a large copper ball, eight inches " in diameter (insulated also) so as to stand exactly at " half an inch distance both from the knob and the " point. The jar being fully charged, I delivered it upon " the copper ball by my discharging rod, whence it " leaped to the knob, which was three quarters of an " inch in diameter, and the jar was discharged by a loud " and full explosion, and the chain was very luminous." Phil. Transf. vol. LXIV. p. 136.

It must be obvious to any careful electrician, and Mr. WILSON had in his answer observed, that an experiment thus loosely and unphilosophically made, was not greatly to be relied upon; because two chains being made use of,

one to connect the ball, and another to connect the ſharp point with the coating of the phial, a different number of links, or different degree of tightneſs in the two chains, would produce a difference in the reſult: for it bing a known property of electricity to paſs moſt readily where it has the feweſt and the ſmalleſt intervals to leap over, the exploſion would naturally paſs that way, where the links were drawn tighteſt; or if both chains were left looſe, as from the plate they appear to have been, then where they were feweſt in number, inſtead of being determined by the circumſtances of bluntneſs and ſharpneſs.

It was however poſſible, that Mr. HENLY might be right in attributing it to the ſharpneſs of the point; and therefore, in order to ſettle this doubt, I deſired that the experiment might be tried over again in ſomewhat different circumſtances. Mr. CAVALLO accordingly tried it in the following manner. Upon an inſulated ſtand he placed a ball, about $\frac{6}{10}$ths of an inch in diameter, and a ſharp point, directly oppoſite to the place where a Leyden phial, when charged, was to be ſet down. Both of theſe, the ball and point, were connected to his diſcharging rod by copper wire. He then took the phial, which held about a quart, and having charged it, ſet it down before the ball and point, took his diſcharging rod, and completed

completed the circuit. After the explosion the distances were measured. In the experiments which I saw the phenomena were as follows: in the first, the distance of the ball was $\frac{19}{20}$ of an inch, that of the point $\frac{26}{20}$; in the second, distance of the ball $\frac{16}{20}$, of the point $\frac{27}{20}$; in the third, distance of the ball $\frac{15}{20}$, of the point $\frac{26}{20}$; in the fourth, distance of the ball $\frac{18}{20}$, of the point $\frac{29}{20}$. In all these experiments the point, though considerably farther off from the coating of the phial, was struck in preference to the ball. In a fifth experiment, when the point stood at double the distance, the ball was struck, and not the point.

The result of these experiments being so widely different from the result of that made by Mr. HENLY, is a clear proof that he formed his conclusions too hastily, having attributed to sharpness and bluntness a phenomenon caused by the unequal resistances of the chains. As this experiment may be made with almost any machine, those who do not chuse to repeat it will have no right to plead the want of a sufficient apparatus, and must look out for some other reason to evade the force of it.

I come now to consider more particularly the practical question, whether the sharp-pointed or the blunt conductors are most proper to be affixed to buildings, as

preservatives

preſervatives from lightning. And here it is neceſſary to obſerve, that buildings may be expoſed to a ſtroke of lightning in ſeveral different ways. The lightning which, to avoid prolixity, I ſhall only ſpeak of as poſitive electricity: the lightning, I ſay, may accumulate directly over the building; or it may be brought towards the building by a ſmall cloud fetching it in ſeveral ſucceſſive trips from a large cloud at ſome diſtance; or a large electrified cloud may be carried rapidly towards it by the wind: a circumſtance this by no means rare, there being no leſs than four inſtances of it upon record in the Phil. Tranſ. vol. XLIX. p. 16. and p. 309. vol. LXI. p. 72. and vol. LXIV. p. 351. In the firſt of theſe ſuppoſed caſes a ſharp-pointed conductor might poſſibly drain the cloud of its lightning as faſt as it began to accumulate, and thereby prevent any exploſion whatever. In the ſecond, as the cloud, by ſuppoſition, not being driven in one direction by the wind, could not move with any remarkable velocity, it is reaſonable to imagine, that in this caſe alſo there might be no exploſion; and that the electricity of the larger cloud might be gradually exhauſted. But if, according to the third ſuppoſition, a cloud of great extent and highly electrified ſhould be driven with great velocity in ſuch a direction, ſo as to paſs directly over the ſharp-pointed conductor, there can

be no doubt but that ſuch a point, from its ſuperior readineſs to admit electricity, would take the exploſion at a much greater diſtance than a rounded end, and in proportion to the difference of that ſtriking diſtance would do miſchief inſtead of good.

But perhaps it will be ſaid, that every ſtroke of lightning falling upon a ſharp point is previouſly diminiſhed by that point, and therefore may more eaſily be tranſmitted through the conductor, than when it falls undiminiſhed upon a rounded end. Upon this ſuppoſition I muſt obſerve, that it not only contradicts Mr. WILSON's experiments at the Pantheon, but alſo Mr. HENLY's expement already referred to in this paper, where the fire flew to a very taper point, and melted the end with a ſtrong and loud exploſion. So alſo the ſharp-pointed conductors affixed in America to the houſes of Mr. WEST, Mr. RAVEN, and Mr. MAYNE, do not *ſeem* to have diminiſhed the force of the exploſion, if we may judge from the violence of its effects as related at large in Dr. FRANKLIN's works. It ſhould ſeem, therefore, that the power of diminiſhing a ſtroke, like that of preventing it, is only contingent, and depends, as we ſaid before, upon the degree of velocity with which the lightning moves.

The

The ſum of the whole is, that conductors, terminated by ſharp points, are ſometimes advantageous, and at other times prejudicial. Now as the purpoſe for which conductors are fixed upon buildings is, not to protect them from one particular ſort of clouds only, but, if poſſible, from all, it cannot ſurely be adviſeable to uſe that kind of conductors, which if they diminiſh danger on one hand, will increaſe it on the other. It is the duty of a pilot to keep out of the way of rocks; but it is alſo incumbent upon him, in avoiding the rock, not to take ſo large a compaſs as to run his ſhip upon a quickſand.

When I ſay that ſharp-pointed conductors may in ſome caſes diminiſh danger, I ſpeak of them, perhaps, rather too favourably: for their power of ſtealing away the electric fluid being confined to caſes where the accumulation is ſmall, it follows, that they only operate where their operation is not wanted. The caſes againſt which we wiſh principally to provide, are the exploſions of extenſive and highly electrified clouds; and here we have ſeen, that blunted ends, as acting to a much ſmaller diſtance, are entitled to the preference.

If it be admitted, that ſharp-pointed conductors are attended with any, the ſlighteſt degree of danger, how much muſt that danger be augmented by carrying them

 high

high up into the air, by fixing them upon every angle of a building, and making them project in every direction? Ought this to be adviſed while there is ſtill a doubt of the poſſibility of their doing miſchief? And can the committee, therefore, be perfectly juſtified for giving ſuch a decided preference to the uſe of ſharp conductors, in defiance of numerous experiments, not one of which they have attempted to controvert?

I have now done with the report of the committee, and ſhall next proceed to enquire whether, as ſome gentlemen apprehend, the termination of conductors is a matter of indifference: but this I muſt reſerve for the ſubject of a future paper.

A P P E N D I X.

I think it neceſſary to apprize the reader, that the foregoing remarks were drawn up before Mr. NAIRNE'S paper appeared, and are therefore to be conſidered as relating only to his experiments.

I muſt at the ſame time obſerve, that, in ſtating the different circumſtances in which lightning may be collected ſo as to affect a building, I have ſuppoſed a caſe which poſſibly may never exiſt. I have ſaid, that lightning may accumulate directly over a building, and admitted, that in that caſe the cloud might be drained of it by a pointed conductor. Now we have no evidence, that the accumulation of lightning is confined to a ſingle cloud, or ſmall circumſcribed ſpot in the heavens. On the contrary, the numerous exploſions, which in moſt thunder ſtorms happen nearly at one and the ſame inſtant, rather lead us to imagine that a great part of the horizon is at thoſe times full of lightning, and therefore incapable of being drained. I would therefore wiſh to have my ſuppoſition underſtood as a mere imaginary ſuppoſition, for the ſake of rendering the argument more perſpicuous, and not as the admiſſion of a real fact.

Laſtly, I beg leave to correct an expreſſion I have uſed with reſpect to pointed conductors, that they only operate where their operation is not wanted. Now this is not accurately true: for if by operating upon a quantity of electricity too ſmall in itſelf to do miſchief, they prevent its growing to a great and dangerous quantity, this would, as far as it goes, be a very conſiderable advantage.

I ought

I ought therefore to have ſaid only, that pointed conductors afford no protection where the danger is great and imminent, and only obviate that which is diſtant and problematical; and that theſe laſt are not the caſes againſt which we principally wiſh to provide.

March 18, 1779.